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errors due to misapprehension; but to charge him with neglect and wilful misrepresentation of another's views involves a presumption of motives which, I trust, are not common among students of science. I have the highest regard for Professor Morse personally and for his valuable and painstaking work in Japan, not only upon this subject but upon others, and I certainly would not willingly misrepresent his views nor disregard them. He will no doubt have observed that this part of the subject is treated in a much briefer manner than might have seemed desirable, otherwise I do not think he would have found any cause for complaint.

ROMYN HITCHCOCK.

The Woodmont, Washington, D.C., Sept. 12.

On Biological Nomenclature.

PROFESSOR UNDERWOOD'S article in *Science* for Aug. 26 calls for a general expression of views on this subject. The article above referred to was written from the standpoint of the botanist, while the present one will be perhaps more from a zoölogical standpoint. The writer, however, recognizes no distinction between the two, and firmly believes that the system of nomenclature should be absolute and uniform for all branches of biology. Absolutely the same rules should be recognized throughout the departments of botany and zoölogy, and these rules and regulations ought to be speedily decided upon by a congress of the leading biologists of the world, to which every country and organization so interested should send delegates. In the meantime every one follows his own particular ideas in regard to the matter, which may be either right or wrong.

I desire here to express my unprejudiced but very decided views on the seven questions which Professor Underwood puts, and will preface them with the remark that in no case can the name of the original erector and describer of a genus or species be separated therefrom without gross injustice.

1. Shall there be an initial date in nomenclature? Let us by all means recognize the validity of the first names proposed when accompanied by a sufficiently recognizable description and not preoccupied. In some cases, as with many of the older authors, descriptions must be recognized which would not be considered sufficient at the present day.

2. Shall names long used be laid aside when claimed for other plants [or animals] on grounds of strict priority? They should, when it is unmistakably evident that the original describer so intended.

3. Shall "the first name under a genus" hold against a previous specific name? By no means. The specific name first proposed should, coupled with the name of its original describer, follow the name of whatever genus it may be finally relegated to.

4. Shall varietal names have priority over established specific names? Yes, but with the name of the original proposer attached. I do not agree with Professor Underwood on this point, but believe that varietal names lay claim to the same priority as specific names, *when they are found to be valid.*

5. Can inappropriate names be cancelled on that ground alone? They cannot with any degree of justice.

6. How far has a later writer a right to correct names previously established? He has no right whatever to in any way change the spelling of a name from what was intended by the original describer. If by a typographical error the name was printed wrong, and the author corrects it later in print, his correction should be accepted. I am strongly in favor, however, of beginning *all* specific names with small letters, whatever their origin, and making all compound specific names into simple terms by writing them with the hyphen dropped. I would write *Brevoortia idamaia* Wood, or *donnellsmithii*, or *mariaewilsoni*, to use Professor Underwood's examples. I have no right to change the endings in any way whatsoever, neither have I the least right to supply a syllable apparently omitted, judging from the derivation. I would not consider that I had the power to elide or supply a single letter, if by such act I changed the term from what was originally proposed and intended by its describer. My conviction is that, except in manifest errors of *typography*, names should be let alone. Errors of orthography may be left to stand.

7. What credit should be given for generic and specific names? Write the name of the author of the specific name, *without* parentheses, whether there have been a dozen transfers or none at all to a new genus. There is no necessity whatever for shedding glory upon the one who made the transfer. Usually he erects a new genus to accept the transferred species, and the fact that his name will go down the corridors of time coupled to the genus he erected is glory enough. He has no right whatever to the species. Even if he does not erect the genus, he certainly has full credit in the literature for making the change, and the act does not demand recognition in the system of nomenclature itself.

I would write *Metzgeria pubescens* Schrank, to use the example given in the article referred to, and make no more ado or trouble about it. This signifies *always* that the authority named described the species originally and originally proposed that name. The founder and date of the genus can be ascertained by referring to any monograph. The generic conceptions of the original authority should not enter into consideration at all.

As to the question of "once a synonym, always a synonym," I believe in the negative. If a form, which had been described and then thought to be the same as some other species, is later proven to be a valid species, the name originally proposed should stand.

Generic names should not agree too closely in orthography. I should say that *Richardia* ought to preclude *Riccardia;* certainly *Cæsia* should preclude *Cesia.* I do not think that different derivation, or original meaning, presents any excuse for similarity of terms. The difference should be sufficient to preclude any possibility of error on the part of a student unfamiliar with both terms. I believe also that a generic term already used in botany should not be proposed in zoölogy, and *vice versa.* I would be cautious about changing those which have already been of long standing, however.

Lastly, specific names should never be capitalized or written with a hyphen; and no comma should be inserted between the specific name and its authority. It would be a great boon to biologists if absolute uniformity could be infused into the system of nomenclature.

C. H. TYLER TOWNSEND.

New Mexico Agricultural College, Sept. 1.

Grand-Gulf Formation.

I HAVE read with great interest recent contributions to the literature of the Grand-Gulf formation, including Professor Hilgard's valuable paper in the *American Journal of Science* and Judge L. C. Johnson's letter in your last issue. As I have recently been summarizing our knowledge of the Post-Eocene Tertiary (to appear shortly in Bulletin 84, U. S. Geological Survey, which is already in type) I am moved to add a few words in regard to the subject for your columns, which I have already expressed in correspondence with several of those interested.

At the time of the Grand-Gulf sedimentation the lower valley of the Mississippi was already the theatre of estuarine conditions and operations, which date to a very ancient geological time. Toward the end of the Chesapeake or newer Miocene epoch this gulf extended far into the interior, its south-eastern point of entrance being somewhere in the meridian of Mobile, or between Mobile and the Appalachicola River. The embayment, which I have called the Gulf of Mississippi, received an immense drainage, corresponding to that of the whole Mississippi valley and perhaps that of the upper lakes of the present St. Lawrence system. The operations in progress consisted in the transfer of material from the elevated interior to this gulf by the medium of the drainage, and in all probability a gradual or intermittent shifting of level as weight was removed from the uplands and deposited beyond the shore-line. The shallows, as I conceive it, sank and the interior rose, thus preserving a sort of balance, and there is some reason to suppose that a specially important movement took place at the end of the Grand-Gulf epoch, by which the more energetic degradation characterizing the Lafayette epoch was inaugurated, the Strait of Georgia closed, and the previously existing islands of central Florida were joined to the mainland. I agree entirely with Hilgard's view that elevation was essential for the geological operations which are recorded in the stratigraphy of these two epochs.

The Grand-Gulf strata show gravels, sands (now frequently

converted into quartzite), and clays. They were laid down in water which was too brackish at times for the establishment of a fresh-water fauna in the estuary and too fresh for a marine fauna. In short, the conditions were those of an estuary during a period of rather rapid sedimentation. This estuary probably was, as many southern estuaries are now, defended from the sea by low bars or sand islands, on the seaward side of which a marine, probably Chesapeake, fauna flourished, whose remains are now buried 700 to 1000 feet below the level of the Gulf of Mexico. On the shores grew palmettos, and drift-wood in abundance brought down by the rivers was strewn upon them. I regard it as likely that part of the gravels bored through by artesian wells, in the axis of what was the Gulf of Mississippi, are referable to an earlier period than that of the Grand-Gulf epoch, since the same processes were at work there throughout the whole of the Miocene. Coëval with the sediments of the Grand Gulf were marine deposits along the shores of the Gulf of Mexico, both east and west of the entrance to the Gulf of Mississippi. As the erosion of the land became more complete the slope of the drainage became less, the currents slower and the sediment finer and lighter, fine sand and clay replacing the gravel and coarser material of the earlier part of the epoch. In short, the clays to which Johnson has applied the name of the Pascagoula formation, began to be laid down, the sea was less energetically pushed back by the outflowing river-waters, and the conditions became more favorable for the establishment of a brackish-water fauna.

The word formation has been used very loosely in American geological literature. In the sense in which we use the term for the Chesapeake Miocene, or the Grand Gulf, or Lafayette rocks, I conceive that these clays do not constitute a formation. They really represent for me a phase, the latest and most gentle, of the Grand Gulf, which is represented by the sands with palmetto leaves above the Chesapeake strata in the section at Alum Bluff on the Chattahoochee River. We may, slightly modifying Johnson's term, refer to them as the Pascagoula clays.

A correction is also required in the definition of these clays, or rather the fauna they contain. It is not, as supposed by Johnson, a marine fauna. All the species are or may be a part of a strictly brackish-water formation. The collections of Johnson, as well as material from the Mobile well, have been in my hands for study. The fauna comprises a large oyster, a small *Gnathodon*, which I have described under the name of *G. Johnsoni*, a small *Mactra*, also found in the Chesapeake Miocene, fragments of a *Corbicula*, and a *Hydrobia*, which I have named *H. Mobiliana*. The supposed *Venus* of which Judge Johnson speaks is the young of the *Gnathodon*. All these species are characteristic of estuaries. and will be discussed in my "Tertiary Mollusks of Florida," of which Part II. is now printing. The depth at which this fauna is encountered in the Mobile well is 735 feet, which gives an average dip from the locality near Vernal, Miss., where it comes to the surface, of about 25 feet to the mile; which corresponds very well to the dips of other strata of the Tertiary, which have been similarly traced. We are under serious obligations to Judge Johnson for the material he has so assiduously collected and which has helped so much to determine the geology of our southern tertiary formations.

WM. H. DALL,
Palæontologist U. S. Geol. Survey.

Washington, D. C., Sept. 13.

European Origin of the Aryans.

REFERRING to Dr. Isaac Taylor's letter in *Science*, Sept. 9, I must say that I cannot conceive how he can make the statements it contains, if, as he alleges, he has "carefully read" Omalius D'Hallow's writings.

Dr. Taylor's words are, "The comparatively modern theory that the Aryan race originated in the highlands of Central Asia, a theory of which D'Halloy does not seem to have heard." Now, in the article published in 1848, D'Halloy has these words: "On a voulu tirer la conclusion que ces langues (indo-germaniques) derivaient du sanscrit, et que tous les peuples qui les parlaient etaient originaires de l'Himalaya, deux propositions qui sont loin d'être incontestable."

As if this was not enough to make it clear as to what theories he was attacking, he specifically states in a note to page 19 of his "Eléments d'Ethnographie," referring to this article in the Bulletin of the Belgian Academy, that it was directed against the linguists who derived the modern European languages and peoples from Central Asiatic ancestry; whereas it was his view that the ancient Persian and Indian tongues were imported from Europe into Asia.

I imagine that if Dr. Taylor had not had before him the "necessity of modifying former [printed] statements," he would not have overlooked this positive testimony by Omalius to himself.

Media, Pa., Sept. 12. D. G. BRINTON.

The English Sparrow and Other Birds.

MY experience with the English sparrow accords with that of your correspondent X. in your issue of Sept. 2, 1892. Before this sparrow came and multiplied largely, my lawn was populated with cat-birds, red-birds (Cardinal grosbeck), robins, doves, blue-birds, yellow-birds, tomtits, chipping sparrows, wrens, etc.; but now the English sparrow has full possession of the entire premises. Now and then a cat-bird or a red-bird slips in as if to see whether he may again bring his family to their old umbrageous quarters, and to the rations which were provided for their support; but he is not reassured, and soon disappears.

The fecundity, energy, and perseverance of the little vandals are amazing. When the small fruits are abundant it requires a week of active shot-gun work to make them even cautious in visiting the fruit-garden. Some of them last spring took a notion to establish nests on the tops of window-shutters which opened under projecting eaves, and although their nests were swept off almost daily, they immediately began in each case to rebuild on the same spots, and continued this for at least a fortnight. In their nesting, as in some other things, they display more perseverance than discretion. The cats found that they were building in considerable numbers in a large hay-loft, and suppressed many a germ of mischief. The sparrows sometimes swarm like flies in the stable, where they will enter the troughs of horses, cows, and pigs whilst the animals are feeding.

I no longer shoot owls or hawks, but give them a welcome, and every cat and nest-hunting boy has the freedom of my premises.

Lexington, Va., Sept. 12. W. H. RUFFNER.

BOOK-REVIEWS.

Annual Report of the Board of Regents of the Smithsonian Institution to July, 1890. Washington, Government Printing Office, 1891.

THE Smithsonian Report for 1890 contains: First, the proceedings of the Board of Regents for the session of January, 1890; second, the report of the executive committee exhibiting the financial affairs of the institution, including a statement of the Smithson fund and receipts and expenditures for the year 1889–1890; third, the annual report of the secretary giving an account of the operations and condition of the institution for the year 1889–1890, with statistics of exchanges, etc.; fourth, a general appendix comprising a selection of miscellaneous memoirs of interest to collaborators and correspondents of the institution, teachers, and others engaged in the promotion of knowledge. This volume is also profusely illustrated, adding greatly to its value and interest. Among the illustrations are maps of the National Zoölogical Park; maps of the Niagara River; maps of Central Africa, before and after Stanley; pictures illustrating primitive urn burial, the age of bronze in Egypt, specimens of quartz fibres; and many others too numerous to mention in detail here.

The object of the memoirs included in the general appendix is to furnish brief accounts of scientific discovery in particular directions; occasional reports of the investigations made by collaborators of the institution; memoirs of a general character or on special topics, whether original and prepared expressly for the purpose or selected from foreign journals; and briefly to present (as fully as space will permit) such papers not published in the Smithsonian Contributions or in the Miscellaneous Collections as may be supposed to be of interest or value to the numerous correspondents of the institution.